





Miner (T.) & Tully (W.)

AN

# EXAMINATION

OF THE

## STRICTURES

IN THE

NEW-ENGLAND JOURNAL FOR OCTOBER, 1823,

AND IN THE

NORTH-AMERICAN REVIEW FOR OCTOBER, 1823,

ON

ESSAYS ON FEVERS, &c.

Box 1. ✓

BY

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AND

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"TANTÆNE ANIMIS CŒLESTIBUS IRÆ?"

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MIDDLETOWN, (CONN.)

PRINTED BY E. & H. CLARK.

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1823.

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M664e  
1823

Telw# 3327, 20.6

*Extract from Essays on Fevers, &c.*

It is proper to notice a common error, which is prevalent among a certain good sort of people, who are little acquainted with medical subjects. They affect to consider, that there is some *truth*, and some *mistake*, on either side, which physicians have taken, in the management of modern Typhus. The fact is, that no compromise is admissible. It is a question of guilty, or not guilty—a dispute respecting *fundamental* principles and facts. Do Spotted-fever, and other *sinking* diseases [which are attended with a dangerous deficiency of vital power, through their whole course] ever admit of depletion, or reducing agents, in any stage? This is an all-important point. There is no question, that evacuations are often indispensable, in some typhoid diseases; but are they indicated, because they reduce, or because, while removing a particular symptom, their counteraction overbalances their reducing effects? Every human being is liable to mistake, in the application of true principles; yet such principles are as firm and immoveable, as the rock of ages. Was the Spotted-fever of Hartford to be cured by depletion and reduction? Did the Yellow-fever appear in Middletown, in 1820? Does regular Typhus admit of resolution? Are our late Fevers phlogistic, in any stage? There can be no compromise, no splitting the difference, upon such questions. The maxim *in medio veritas*, however applicable it may be, in many cases of *practice*, has no bearing upon questions of *mere fact*, and upon *fundamental principles*.

“A physician, therefore, who is ever boasting of his candour upon these essential points, or who values himself for having no decided opinion upon such vital questions, or upon pursuing an intermediate course, can be considered in no other light, than as a timid and inert practitioner; or as an artful, time-serving man, who is ever ready to sacrifice the life of a patient, for the sake of preserving a little temporary popularity.

“When important facts and fundamental principles are concerned, timid friends, who are afraid of offending by taking a decided part, do more to injure the cause of truth, than its open enemies. We know, on what ground to meet the attacks of the latter; whereas, the former are ever tantalizing us with private approbation, without daring to give us open support. *Non talibus auxiliis.*”

## EXAMINATION, &amp;c.



*"We know, that though the evidence of a particular truth be so glaring, that it forces itself upon the understanding, and all the sophistry of passion and interest cannot withstand it, yet if this truth be of a painful and humiliating kind, the obstinacy of man will often dispose him to resist its influence, and in the bitterness of malignant feelings, to carry a hostility against it, and that too, in proportion to the weight of the argument, which may be brought forward in its favour."*

THE medical school of Edinburgh, at the time of the death of Cullen, was undoubtedly the best institution of the kind, in the whole world. The collision between Cullen and Brown, led to close and accurate observation, and served to develope and establish the principles of medicine, upon a firmer foundation, than they had before rested upon, from the time of Hippocrates. No man can with justice make any pretensions to medical science, who is unacquainted with the principles and practice of that period. Within the last twenty years; however, there has arisen a class of physicians, who appear to be almost entirely ignorant of these principles, and this practice; or if they have ever read the authors of that day, it has been in such a superficial manner, and under such biasses and prejudices, that they have received no practical impressions from them. Indeed, so much, and so exclusively, have some late superficial and mushroom writers, engrossed the attention of the medical public, that it is apprehended, many of the older physicians have nearly forgotten the principles, as they obviously have, the practice, of the school in which they were educated. The indolent doctrine, that diseased action is a unity in kind, and varies only in degree, seems, so far as respects fashionable practice, to have usurped the place of every other. It is high time that the spell should be broken. An indolent, an indiscriminate, a pernicious practice, has already been too long tolerated. The current has set long enough in such a course; but from the length of time that it has been moving in one direction, and the momentum which it has acquired in its progress, it must be met resolutely, in order to produce any effective resistance. A mere tame statement of facts and principles, can be expected to make no serious impression, in opposition to such measures as have been, and still are, employed, for the propagation and support of the errors in question. But there is another point of light, in which this subject is most strikingly important. Where the health and lives of so many of our fellow-citizens

are involved, what conscientious man would not feel himself in duty bound to make some efforts for a reformation? Who, that can wield a pen, can refrain from making animadversions, when the indiscriminate practice of Hamilton, Hey, Clutterbuck, Armstrong, and Welsh, with many others in their train of lesser note, is seriously inculcated, for *our* diseases? The worst of the matter is, that the doctrines of these authors were unfortunately broached, and made popular in the United States, at a period, when *our* diseases were daily becoming more asthenic; so that when low Nervous and Spotted-fevers, typhoid Pneumonia, &c. became epidemic, the younger physicians, who had just emerged from the schools, had none but such blind and visionary guides, to direct and distract them. Much of the mortality, which has attended our malignant diseases, during the last fifteen years, must doubtless be attributed to this cause. "*A cet égard, point de faiblesse, point de concessions au nom, à la considération personnelle.*" Who, that has any regard for sound principles, can stand by calmly, and see our medical students bewildered in the mazes of visionary hypothesis, and absurd practice, which must inevitably result in skepticism and imbecility on the one hand, or rashness and hazard on the other; and this, even without the smallest clue to guide them? Do we not every day read statements of practice in Croup, Pneumonia, Phthisis, Rheumatism, Malignant-fevers, Colic, &c. in our first medical journals, where the editors do not choose to make note or comment, though it would disgrace a medical tyro?

Whoever has been familiar with the foreign journals, is well acquainted with the attempts that were made in the year 1818, to fill all the French medical professorships with anatomists and surgeons, to the exclusion of those, who had devoted their principal attention to physic. This disposition of the anatomists and surgeons, to usurp the place of the physicians, and to consider a minute knowledge of their branch, as essentially adequate to medical practice, has not been confined to Paris. The same notions have prevailed in our country, from an earlier period. It is not uncommon in our medical schools, for the lecturer on the practice of physic (the most important place in such institutions) to be little more than an operative surgeon; and so incorrect an estimate do such men sometimes form of their own qualifications, as to insist on taking the preference, in such a station, to ingenious, learned, and experienced bed-side practitioners of medicine. To such an extent has this undue preference to surgery been carried, that many of the medical journals of the present day, are principally occupied by surgical cases, or by minute and trivial pathological speculations and inquiries, founded on slight appearances on dissection, and that often, after the most preposterous practice; or on barbarous and puerile experiments upon living animals. It is confidently believed, that an experienced physician, may regularly read a dozen of the most popular works of this kind, with scarcely acquiring, in the



course of a year, one new, useful, practical idea, except as respects the auxiliary branches. Even the multiplicity of new facts on *Materia-medica*, are of very little importance, from a deficiency of all accurate observation, with respect to the most important circumstances of the diseases, in which the articles have been employed. The cases which are detailed, are so generally and loosely stated, and with such a deficiency of data, as respects the true indicating and contra-indicating symptoms, that no accurate judgment can be formed, either of the correctness of the pathology, or the propriety of the practice. From such considerations, we still contend, notwithstanding all the self-sufficiency, self-flattery, and self-puffing of the present period, that the pure principles and practice of physic have been, of late, essentially deteriorated, and that acute diseases in general, are by no means so well understood, or so well treated *now*, as they were thirty years ago.

In fact, such and so strong is the current of medical opinion, in favour of the fashionable, crude, hypothetical, and indiscriminate doctrines and practice of the present day, and so timid, and so fearful of their popularity, are the editors of many of the journals, or so completely have they become converts to the doctrines in question, that it is doubtful, whether there is any considerable proportion of them, in the United States, who would be willing to publish an unequivocal refutation of the current opinions. Many private practitioners of talents and worth, who have been convinced by ample clinical experience, of the gross errors of the host of the indiscriminately antiphlogistic writers and their followers, have not independence and courage sufficient to come before the world, with their convictions, and the details of the long series of careful observations, which have led them irresistibly to their present conclusions. They very well know, that an immediate clamour would be excited, and every attempt made, to injure their reputations and business; and by those very men, who are ever ready to raise the cry of abuse and personality, against every one, who ventures to dissent from their opinions, or oppose their peculiar dogmas. "*They want that boldness of argument, which the merits of the question entitle them to assume.*" "*They ought to maintain a more decided front to their adversaries.*" There are even some, to our personal knowledge, whose practice is judicious and correct, who will nevertheless, both in conversation and in print, recommend practice, that is diametrically opposite to their own.

That *our* work, written with such views, and under such circumstances, should meet with strong opposition, was to be expected; but, that there should be an attempt to silence its authors, without giving them a hearing, could not have been anticipated. That such an attempt has however been made, we shall show, in the course of our remarks. Nevertheless, we cannot for a moment indulge the idea, that American readers will commit their judgment and opinions, to the guidance of any

man, or body of men, without exercising the right of investigating, and deciding for themselves. "*Vir bonus, quod honeste se facturum putaverit, faciet, etiamsi laboriosum erit: faciet, etiamsi damnosum erit; faciet, etiamsi periculosum erit.*"

The reviewer in the New-England Journal, begins with a most obvious misrepresentation, as he says, that the two parts of our work, are entirely unconnected. On the contrary, they are as closely connected, as if they had been written by the same hand. There is not one single Essay in the whole volume, that is not important for the illustration of some of the rest. All the Essays of the first part, after that on the resolution of fevers, are necessary for a clear understanding of that, or at all events, to prevent misapprehension. Four of them were originally written in the form of notes to the first. The second part of the volume is principally, a practical illustration of the principles inculcated in the first. It is true, those who are yet familiar with the doctrines of Cullen, and his immediate successors, would find no difficulty in understanding any part by itself; but it is believed, that these principles have of late been so much neglected, and the most popular writers and instructors of the present day, have laboured so much, and so successfully to obscure them, that to the younger classes of the profession, the second part must have been nearly unintelligible, without the first. The authors of the work never dreamed, that it *could* be considered otherwise, than as a whole. The truth is, that for several years, they have been engaged together, both in practice and study, and there is no principle advanced in the work, which had not long been a subject of mutual observation, investigation, and discussion. As respects authorship, it would have been immaterial, with more than one of the Essays, which name had been prefixed. Any pretensions, therefore, to separate the two parts of the volume, or to countenance one, while the other is condemned, cannot fail to expose the reviewer, and prove, that the intrinsic merits or demerits of the work had nothing to do with his decisions.

As the reviewer supposes, we readily admit, that the authors had the strongest reason to anticipate an unfavourable reception, from a *certain class* of physicians; and among those who are familiar with the late indiscriminate antiphlogistic writers, there can be no sort of difficulty, in accounting for the "*foreboding*," which the reviewer mentions. "*C'est donc une chose reconnue, qu'attaquer une réunion de savans, c'est engager un combat inégal; c'est irriter un guépier.*" We understood human nature too well, ever to flatter ourselves, that we should be able to convince those, who had long adopted gross errors, or committed themselves by publicly defending them. In such, truth and reason are at variance with their personal interests and reputations, and the lives and health of the community, as with Sangrado, are of secondary consequence. These are heavy charges, yet they can be abundantly established. We



had no expectation of making a permanent impression upon that description of physicians, against whom these allegations are so frequently, and so confidently made. We well knew, that no man was ever convinced, by attributing to him improper motives, however justly he merited having them exposed. The more he deserves censure, the more callous are his feelings, and the less likely is he to be reformed. But all men are not like mere theorists, and cold-blooded reviewers, who have set truth and candour at defiance. The community in general are wedded to no system. They have the highest concern, to know the principles and practice of those, who assume to themselves the prerogative of guiding and regulating the public taste and judgment, as well as of those, who profess to be guardians of their lives and health. However, we have not, by any means, cast the first stone. The attack did not begin, on our side. What, we inquire of the reviewer, has been the spirit manifested by those American writers, who maintain the phlogistic character, and inculcate a purely antiphlogistic treatment, of Spotted-fever, and Pneumonia-typhoides? What the civility or the candour, which they have manifested to those practitioners, who were educated in the school, and continue in the doctrines of Cullen, and his immediate successors? Who *first* brought the charge, that the severity and mortality of many of our late epidemics, have been owing to the practice, rather than the malignity of the disease? What was the usage, and what the ingratitude, that the physicians of Hartford experienced, for their philanthropic and skilful exertions, during the prevalence of Spotted-fever, in 1809, the most deadly typhoid disease, that ever appeared in New-England, of which there is any record? What were the decisions of the medical men, of *some of the large towns at a distance*, where the disease had never appeared, respecting the practice of the Connecticut Physicians, some of whom have had the management of two thousand cases of the disease? What was our own reward, for hazarding our lives in the Yellow-fever of Middletown? What, this very year, has been the usage of the physicians of Berlin, Connecticut, and the adjoining towns, notwithstanding their success has probably been much greater, in proportion to the number and severity of the cases, than has ever before been witnessed? Or to look across the Atlantic, how does Dr. Welsh treat his illustrious predecessors in Edinburgh? There has never been any scruple of raising the cry of Brunonianism, and indiscriminate stimulation, or of charging us with mal-practice. Our opponents can themselves violate the confidence of medical consultation, they can dexterously evade, or even defy, the regulations of the medical police, they can distort our principles, and most grossly misrepresent our practice; and yet, when any of their doctrines are controverted, when the results of their practice are scanned, when their management and intrigue are exposed, this is slander and persecution! Thus it appears, that in censuring and accusing us of needless asperity, they convict themselves.

When next they demand justice, let them come into the court with clean hands. "*Hypocrita, ejice primum trabem de oculo tuo.*" But though so much abuse and obloquy, have, in some publications, been heaped upon those, who have practised, in such low typhoid diseases, upon a counter-acting and supporting plan, yet, our brethren at a distance from the scene of action, can have but a faint idea, of the *private* bitterness and malice, which have been so unsparingly vented. The review, now under consideration, is a striking example of the abusive treatment, that a certain class of physicians usually bestow, upon those who do not subscribe to all their dogmas, and will of itself, stand as a decisive proof of the justice of our assertions. The determination to criticize, *without studying the work*, and the disposition to reject its principles, without putting them to the *test of experiment*, may be considered as a fair specimen of the class of writers and practitioners, to which we refer. "*The human mind feels restless, and dissatisfied, under the anxieties of ignorance.*" "*It longs for the repose of conviction; and to gain this purpose, it will often rather precipitate its conclusions, than wait for the tardy lights of observation and experiment.*"

Without an explicit assertion to that effect, the reviewer, by a pretended or real ignorance of many of the facts to which we refer, insinuates, that they are not correct. Is it possible, that he can really be ignorant, that any author has asserted, that the Bark is rarely serviceable in our fevers; that blisters to the head are of no use in typhoid affections of the brain; that Opium, in regular and repeated doses, during the stage of febrile exhaustion, is inadmissible; that the mercurial action, when moderately excited during the preparatory stage, is of no other service, than as a test of the natural mildness of the disease; or, that continued fevers are never cured by resolution? Has he never read the last American edition of Wilson Philip on fevers, of Thomas's practice, or even Welsh on the fever of Edinburgh? The reviewer has placed himself in a complete dilemma, from which no sophistry or equivocation can extricate him. If he is really unacquainted with the common text-books of our medical students, and young practitioners, he is the most presumptuous writer, who ever attempted to direct the public judgment. On the other hand, if from sinister motives, he has intentionally concealed his knowledge of the most notorious facts, no language can be too severe, to stigmatize such a bare-faced prevarication, which it must be in the power of every tyro in medicine to detect.

It would be sufficient for us to stop here, and close with the reviewer's remarks upon our preface. Every candid and impartial man, must at once see, than no reliance can be placed, either upon the accuracy of the statements of such a critic, or the impartiality of his conclusions. After proving him guilty of such gross ignorance, and such palpable misstatements, his remarks and comments, must cease to have any influence,

with those who ever take the trouble of investigating for themselves. Indeed, we *would* stop here, were it not, that too many of mankind, are strongly inclined to take their opinions upon trust, and that slander, however improbable and inconsistent, will injure any reputation, unless it is thoroughly exposed. "*Tardiora sunt remedia quam mala.*" Our friends, therefore, will excuse us, for dwelling more upon this review, than its intrinsic merits, by any means, demand. The object of the critic is evidently, neither the investigation nor the propagation of truth, but merely to prevent, *per fas aut nefas*, the circulation of an individual work, and the diffusion of the principles and practice which it inculcates.

Notwithstanding the critic affects to consider the commencement of the Essay on the resolution of fevers, as a mere rhetorical flourish, we do still contend, that "*in one form or other, febrile diseases destroy much the greater portion of the human race.*" Were the assertion of the critic of any authority, we should be happy when we find, that *he* agrees with us, that the majority of cases may be broken up, in their first stage. His usual ignorance, however, attends him upon this occasion. He would be thought not to know, that a popular professor, who has lectured in various places, and probably had more pupils than any other medical teacher in New-England, has long and indefatigably inculcated, both in his private instructions, and through the medium of the press, the contrary doctrine. He has even had the civility to tell one of us, that our assertion, that we frequently succeed in breaking up Typhus at its access, was a falsehood. Whether he will have the hardihood to repeat this opinion, in his work on Typhus, in opposition to this redoubtable reviewer, is a problem. Thus critics do not better agree with popular instructors, than with authors. The reviewer's remarks upon this subject, are all of a piece with his strictures upon the preface. So far from claiming this, as any discovery of our own, as he asserts, we expressly state, that, "*every powerful agent is occasionally found to break up disease;*" and in another place, we mention the several means, which have been occasionally found to produce this effect. In fact, so careless is he, in his zeal for misstatement, that within a very few pages, he himself observes, that we remark, that bleeding, emetics, cathartics, &c. no doubt occasionally succeed. It is our *process* only, that is original.

The critic, with his usual consistency, has quoted from Dr. Warren, the practice of giving repeated *full* doses of Calomel, in a different variety of Typhus, to prove, that we are not original, in our *method* of slow and moderate purging with *small* doses, in our disease. However, by the omission of the fact, that antimonials and blisters were superadded to this use of Calomel, he has even misstated Dr. Warren. He has further quoted Dr. Warren's use of small and repeated doses of Calomel and Opium, in a different fever from ours, to produce "*copious evacuations from the bowels,*" a practice not confined to the early stages, and not expected to



produce resolution, and likewise the use of "*a few doses of Calomel,*" in the secondary stages, "*to excite the system,*" to prove, that our method of practice, at the access of Nervous-fever, has long been common! The fact, that the reviewer cannot see, but that all this is "*precisely similar,*" is demonstrative evidence, of the superficial manner, in which he has read our work, if indeed, he has read at all, any more of it than he has quoted.

He represents us as maintaining, that "*bleeding, emetics, and active cathartics, are all, not only useless, but dangerous, in the beginning of an acute disease!*" It seems, he did not even read the whole of our preface, as he would there have found, that "*the author readily grants, that there are various plans of medication, which are usually attended with considerable success;*" nor indeed, a considerable portion of the first Essay, in which we speak of abstinence, sweating, cold-affusion, bleeding, emetics, cathartics, narcotics, and factitious strangury, as all having been occasionally successful, in producing resolution, as having sometimes removed every symptom of Typhus, and as having different degrees of merit, and different circumstances in their favour. In the subsequent parts of our work, much pains is taken to specify the particular cases, in which bleeding, emetics, active cathartics, cold-affusion, antimonials, the refrigerant salts, and vegetable acids, are not only proper, but absolutely indispensable. Indiscriminate practice, is the last accusation, that we should ever have anticipated. The whole tenour of the work, is in direct opposition to this very thing; and we repeatedly and constantly urge, that no medicine should ever be given, without a definite and precise object in view. We do not hesitate to assert, that no work in the language, is more precise on this subject. The critic could not have misunderstood us. He must wilfully and wilfully have misstated and distorted our whole meaning.

We are next represented as pretending to cure a disease, which is often fatal in six hours, by retaining Calomel in the stomach and bowels, for twelve hours. He certainly never could have published such a glaring falsehood (we do not hesitate to call things by their true names) had he ever read the work. The cases which are exceptions to our use of Calomel, are repeated again and again, and we expressly say, that "*the occurrence of such extreme cases, is very common of late years;*" and in the preface, it is stated, that "*a primary object of this publication, is to give precise rules, for the administration, the abstraction, and the non-employment of Calomel.*"

The critic next attempts to apologize, for not making any further *analysis* (as he is pleased to term it) of the volume, for not furnishing any *further abstract* of its contents; and his excuse is, that it is not worth the trouble. But, are the subjects of diathesis, type, stage, crisis, pulse, indications, experience, counteracting and coinciding agents, inflammation, &c. even though they might be indifferently treated of, of so little conse-

quence, as not to deserve notice, in a review of this length? It might reasonably be supposed, that a work not worthy of an analysis, or an abstract, would not be worth noticing at all; and yet, so inconsistent is he, as to occupy *twenty-eight* large octavo pages, of a fine type, with misrepresentation, cavilling, invective, and unfair extracts, in reference to this very work. He concludes his apology by declaring, that "*the book is filled with rash and unfounded assertions, extravagant theories, and the most gross and unwarrantable aspersions upon the profession, mixed with a large share of self-complacency and arrogance.*" As these charges rest exclusively upon the credit of the reviewer, being entirely unsupported by any statement of facts, or chain of reasoning, and as we have already shown, how little confidence is to be placed in any of his statements, or even quotations, it might be sufficient to reply, that the work does not contain a single assertion, that is not founded on the amplest observation, or the most conclusive testimony; that not a *single theory* is advanced in the whole book; and that there is not a single charge brought against any individual or body of the medical profession, which cannot be supported by the clearest evidence, and indeed, most of them have actually fallen under our own immediate observation.

The charge of libelling the whole medical profession, is as unfounded, as his other sweeping accusations. The tricks of the venal and ignorant part, it is true, are exposed in the strongest, but at the same time, in the justest colours. Nor are we at all inconsistent, when we caution against the occasional failings of the most able men of the faculty; for practitioners of medicine, in common with every other profession, have their "*esprit de corps*" with their peculiar virtues and defects, arising from the motives and temptations, that are peculiar to the situation, in which they are placed. No body of men, unless they are upon a constant guard, are so apt "*to row one way and look another,*" to talk, and apparently act, with the vulgar, however correctly they may think with the wise. Is it possible, that any proofs can be wanting, to substantiate these assertions? Who has ever read the surreptitious editions of Cullen's clinical and other lectures, and has not seen, that his private practice was much more efficient, than he chose to avow, in his acknowledged works? From unquestionable private authority, we could mention instances of the same kind, even in some of the living authors in London. Among our personal acquaintance, and those whose general practice and views perfectly coincide with ours, in every essential point, there are many, who think it quite inexpedient to defend publicly, the very treatment, which they employ every day, in low, cool, typhoid diseases. We have stated in our work, that in one county of Connecticut, we do not know of a single antiphlogistic practitioner, in the diseases which have prevailed of late; nor, in the whole course of our lives, did we ever know an individual, who was able to treat such diseases with success.



by antiphlogistic means. We have seen many attempt it in the beginning of an epidemic, but like the ingenuous Dr. Miller, whose communication we have analyzed, they have invariably relinquished it, in the progress of every severe disease. These things ought not so to be, in a scientific age. The mystery and legerdemain of the dark ages, are still, by no means, entirely discarded, by some of the ablest members of our body. Notwithstanding these free, but at the same time, literally just remarks, it is obvious, through our whole work, that we are enthusiastically attached to our profession. In the words of one of our reviewers, we do indeed "*love our brethren, with all their vices and follies.*" The decided manner, in which we enforce the necessity of learning, industry, and more especially, *moral integrity*, and in which we expose ignorance, indolence, and all kinds of juggling, can never displease any honest man. None but physicians of the description of our cavilling, prevaricating reviewers, can be offended. This, as we have elsewhere stated, is no more than was anticipated.

We could specify facts enough, to substantiate abundantly, the correctness of our remarks, upon the state of large cities. Some of them must have fallen under the immediate eye of the reviewer. We will ask Mr. Critic himself, whether those who write the most, have the most extensive practice? We would inquire, whether the professor, who has now in the press a treatise on Typhus, has been extensively engaged in family practice, for at least the last ten years of his life? We might further very properly inquire, how many cases, during that time, he has witnessed daily, through their whole progress, from the access to the termination? Happily, in Connecticut, the truth can never be considered as a libel. Whatever time-serving authors may say to the contrary, we know it to be a well substantiated fact, that from the era of Hippocrates, to the present day, no physician ever practised with success, in a low and malignant, cool typhoid disease, in which there was a great deficiency of vital energy, unless he met the symptoms of exhaustion, in a similar manner to ours. Upon this point, we claim no originality. We have only endeavoured to be more precise than our predecessors. Alcohol, Opium, Ammonia, Arsenic, Camphor, Cantharides, Capsicum, and the essential oils, individually, or variously combined, or articles of similar tendency, together with the more permanent tonics, must not only be used, but used with energy, for exciting and supporting the vital powers, or every malignant epidemic, soon becomes a devastating pestilence. Whatever may be said to the contrary by closet men, it is the only successful practice of all ages. We have however, endeavoured to recommend an *early* counteraction, which in a great measure, may preclude, in regular Nervous-fever, the recourse to such Herculean means; and on this important point we think, that we have made great improvement; but, when the exhaustion is excessive, whether it exists when we are first

called, or takes place after the lapse of one or two weeks, every physician is necessitated to resort to them, or his patient dies. The diffusible excitants *must* be used with considerable freedom, in such epidemics, whenever they would be successfully treated. We have observed, that many of the antiphlogistic practitioners themselves, when they use these articles in extreme exhaustion, do it with vastly more freedom than we. There is this prominent difference between us. They treat the stage of re-action in Typhus, as if it were sthenic *inflammation* instead of mere *irritation*, and thus sink their patients so low, as to make much more medicine necessary, in the latter stages, to produce the same effect. Those great physicians who succeeded Cullen, strikingly illustrated these principles, though, as we have said of their master, it is very evident, that they were cautious of entering into minute details. Moore,\* Townsend, and a few others, were perhaps exceptions. The difference between the apparent weight of disease in Europe, and the consequent apparent imbecility of practice, which we stated in the Essays, should also be taken into the account, in order to do perfect justice, to those eminent authors, who so much improved the principles of medicine, and to whom, our profession is so much indebted. These remarks, whatever a captious critic may say to the contrary, are not at all inconsistent with the respect and veneration, which we have ever entertained, and which we have repeatedly expressed in our work, for the *general* practice of Huxham, Cullen, Darwin, Beddoes, Ferriar, Currie, Fordyce, &c. As respects the practice of authors in general, most reviewers and compilers of periodical publications, usually know *experimentally*, whether those physicians who write the most, have commonly the most extensive practice.

One of us is repeatedly accused by this reviewer, of rejecting all regard to symptoms, of giving no descriptions of disease, of making only a few desultory remarks on the general character and type of diseases, and of founding his practice on mere speculative principles. This has just as much foundation in truth, as the rest of the critic's assertions. It is true, Dr. M. does not write upon any particular epidemic, and of course, does not specify the peculiarities of any one; but of the symptoms of simple typhoid diseases in general, *as they exist among us*, and of the peculiarities of the several varieties of these, he has, in the eleventh Essay, given the most minute detail, that is any where to be found. The terms diathesis, with its division into sthenic and asthenic, type, including the nervous and sub-nervous, the putrid and sub-putrid, as well as stage, crisis, &c. are all accurately explained, and not in theoretical expressions merely,

\* Dr. Moore observes, "*As I am told, this part of my work will be exposed to censure, I refrain from mentioning, how large quantities of Wine, I have known patients take with the best effects, in Nervous-fever.*" Surely, even our reviewer will agree that this, at least borders upon an author's concealing the real extent of his practice.

but by actual and minute specifications of that *aggregate of symptoms*, which constitute them. We do not pretend to consider these terms, in any other point of light, than as signs of such aggregates merely, and substitutes for them. In no part of the review, are we more palpably and wantonly misrepresented, than when we are accused of disregarding symptoms. If the critic had merely read us, in the most superficial manner, he could never have ventured, even to make such a suggestion. We again insist, that we are uncommonly minute and precise upon this subject. We repeatedly mention, not only the manner in which symptoms are to be met, as for example, not merely by prescribing to their names; but we show, that by considering them in connexion, the *same* symptom often requires a *different* treatment, according to the diathesis, type, stage, &c. as well as the known obstinacy, and general character of the prevailing disease. We again declare, that there is not a single theory or hypothesis, in the whole of our volume, and we cannot refrain from expressing our astonishment, that any man should have the effrontery and audacity, to make assertions to the contrary, before that public, to which our work is committed. It can be accounted for in no other way, than by what is doubtless the fact, that the reviewer had never read the work, or that he supposed, he could prevent any one else from reading it. Notwithstanding our challenge, he has not even attempted to produce a single fact, in opposition to our statements, respecting the diathesis, not only of our own diseases, but of every wide-spreading and *devastating* epidemic, which has ever occurred. We indeed hoped, that some candid man would take the field in opposition to us, as we are desirous that the subject should have a thorough discussion. It is an all important point, to ascertain, whether those diseases, which we term sub-putrid, are really sthenic in their first stage; or whether most of the subsequent phenomena, which are supposed to support this opinion, are not entirely the results of the most common practice adopted; or, in other words, whether asthenic irritation is not usually mistaken for sthenic inflammation? The facts upon this subject, though long familiar to physicians in this quarter, seem just beginning to attract attention in Europe, as would appear, by some late essays, in the London Medical Repository, on the effects of great loss of blood.

We have, however, principally confined our work, to a variety of *febrile* disease, very different from the sub-putrid, and one, about the asthenic character of which, at least as far as we know, there has never been a reasonable question. We boldly assert, without the least fear of any well supported contradiction, that a vast majority of *our* fevers invariably exhibit, a greater or less degree of deficiency of vital energy, from their very *access* to their termination. As we have so frequently declared, the heat of the surface seldom rises to the standard of health, during the greatest exacerbations. That this is not owing to an oppressed state of the



system, is abundantly evident, by its being accompanied with every symptom, which indicates *primary* exhaustion, as well as by the effects of all sorts of treatment. The difficulty is, that those physicians, who live at a distance, and are conversant only with diseases of the sub-putrid type, or the very mildest of the nervous sort, *can never be made to realize*, the perfect accuracy of our statements. It is a great misfortune, in regard to medical subjects, that the belief of physicians, is too apt to be confined to their own experience, whether it is right or wrong. The strongest testimony, when opposed to their own impressions, has rarely any influence; and when they make trial of any proposed plan, which is in opposition to their own method of thinking, it is usually so imperfectly executed, as to produce no satisfactory result. The instances, in which gentlemen of considerable talents cannot, or rather will not, employ Opium, Cantharides, Arsenic, &c. to any useful purpose, are innumerable. Were it not an invidious task, many names might readily be specified. Any man, who supposes that a malignant disease, or such a one, as from its access, is attended with a *dangerous* deficit of vital power, may be cured by bleeding, or evacuations as such, *can never* have witnessed a low typhoid epidemic, whatever he may imagine to the contrary. Nor, if he has only seen ever so many *light* cases of the same name, can he ever be convinced, that the disease is *necessarily* more dangerous, in a *malignant* form. Occasional sporadic cases, that terminate suddenly, are commonly mistaken for Apoplexy, Hydrocephalus, Tetanus, or more vaguely, for some sort of *Fit*, and consequently afford no *true* experience. Even when such a practitioner resides in the same town, where an epidemic prevails, if his business is contracted or limited, he will give no credit to the statements of others. Many instances in our vicinity, might be specified, in which one or two popular physicians of a town, have had more business in an epidemic, than they could well attend to, while the remainder insisted, that the place was uncommonly healthy. With too many, their own *limited* business, their own *confined* experience, is made the standard, by which the business and experience of others are to be tried. It should, however, be recollected, that the whole of such experience, is of the *negative* kind, and has not the weight of an atom, in the balance of truth.

Had the size of our volume admitted of the publication of all the Essays, which we at first proposed, our reviewer could not have had an opportunity to plead his *entire ignorance* of the whole of the cases of preposterous practice, to which we refer. Nor can he now plead it *in truth*, if he has been in the habit of reading most of the popular journals and treatises of the last twenty years. He must assuredly recollect General Washington's case. Hale's, is to be found in the Philadelphia Medical Museum, volume 2d, pages 7th and onward. La Roche's, in Chapman's Philadelphia Journal, volume 5th, pages 369—382. Johnston's Purpura-hæmorrhagica, as he absurdly calls it, is republished in the New-England

Journal, volume 12th, pages 83, 84; and we would refer him to Kemper's case of Rheumatism, in Chapman's Philadelphia Journal, volume 6th, No. 12, to Welsh on blood-letting, in the fever of Edinburgh, to nearly *two hundred* victims of depletion, mentioned by Dr. Miller, &c. &c. The principal indications, that are supposed to justify this mass of absurd practice, with which not only the preceding cases abound, but which is to be found in almost every periodical publication, have again and again been proved to result from the practice. See, Bonner and Lucas, on the state of the pulse, after profuse hæmorrhage, and Copland on cerebral excitement, in the London Medical Repository.

The critic sneers at our assertion, that those efficient agents of the *Materia-medica*, which from their prominent *operative* effects, furnish accurate *tests* of the extent to which they can be safely employed, do vastly less harm, in the hands of the skilful, than the more common articles, in the practice of the indiscriminate, the immediate tests of which, are less obvious. Though it is impossible to refute a sneer, yet it should be recollected, that a sneer itself refutes nothing; and notwithstanding he winces at our charge of ignorance, if he really misunderstands us, we will repeat it; and if he intentionally misrepresents us, he merits the application of a much severer term. His cavilling suggestion, that some of our doses of medicine are "*rather large*," we have just anticipated, when we stated, that such physicians judge of the experience of others, by their own *limited* practice.\*

\* With respect to the repeated personal insinuations, which are thrown out at a venture, against one of us, that the results of his practice, have had an unfavourable influence, upon his individual fortune, &c. &c. &c. we will observe, that the critic has entirely mistaken his aim. Dr. M. is neither a misanthrope, nor one who has been disappointed in ambition; nor is he at war with any of the Physicians around him. The only professional collisions, in which he has ever been engaged, were with two medical surgeons. For aught that he knows, he is on good terms with every other member of his profession, with whom he is ever liable to come into contact; of which, their repeated and unsolicited marks of confidence and friendship, are the highest evidence. His field of consultation with others, is more extensive, than his greatest ambition ever aspired after. Nor has he any reason to complain of neglect, or disrespect, from a very great majority of the population, with which he is surrounded (whatever exceptions he might make, in regard to a very few individuals) nor has he ever had reason to complain of a want of his full share of the business and emoluments of his profession, in comparison with his brethren in general, in this State. Though fond, almost to enthusiasm, of his profession, still (except that he would wish to be engaged in some useful and humane employment) he is utterly indifferent, whether he is, ever again in his life, called to prescribe for a single patient. Though he entertains the highest regard for the opinions of the discerning and impartial part of the community, yet no man feels, or actually is, more independent of public frowns or applause. This the reviewer might have easily discovered, from the whole tenor of his writings. But, perhaps it may be some gratification to the benevolent critic, to be informed, that whatever may be the fate of our work, it will produce no embarrassment, in a pecuniary point of view, either to the authors or publishers. However, we by no means wish to intrude our private concerns upon our readers, and should not have hinted at them, but for the critic's disingenuous and ungentlemanly suggestions.



The critic appears to be offended at our assertion, that the practice of physic generally, has not been improved, since the time of Cullen, and his immediate pupils and successors; but yet, he does not advance a single argument, or state a single fact, in opposition. We uniformly admit, that the auxiliary branches are much improved, but still, without advantage to the *pure* practice of medicine, especially in fevers. It is well known, that thirty years ago, in serious epidemic febrile diseases, out of those cases which went through a regular course, one in twenty, in family practice, and one in ten, in that of hospitals, was the average loss in England; and from satisfactory information, we venture to say, in this country too. But, what are the results of modern practice, as stated by Armstrong, and others of the same school? From him it appears, that of late, under a plan similar to his, one in about three, dies in hospitals, during the worst epidemics, and one in about twelve, in the mildest ones.\*

We would likewise inquire of our reviewer, how much the proportional mortality of Yellow-fever, has been diminished, within the last twenty years, in New-York, Savannah, New-Orleans, Natches, Pensacola, &c. ? What likewise has been the relative number of deaths, under the fash-

\* It is true, that Welsh's statement, would seem, at first view, to be more favourable to the modern practice; but several things are to be remarked, in reference to his results. 1st. His disease was a very *mild* one, never becoming at all severe, except from neglect, or palpably bad management. 2dly. His numbers include, not only those which went through a regular course, but those which terminated in their early stages. 3dly. Welsh proves too much; for if the moderate mortality in the fever of Edinburgh, is to be ascribed to the treatment, it will go to prove, that the most indiscriminate practice, ever adopted, is the most successful. 4thly. It would show, that the great body of the physicians of Great-Britain, are absolutely bad practitioners; for, Welsh himself says, "*the practice he recommends, though certainly not novel in books, has still a large majority of the most eminent medical practitioners decidedly opposed to it; and a vast number of others, who look to experience, and a kind of universal consent of the profession, as the signal for submitting to trial, a practice which they consider as hazardous.*" And again he says, "*In this city, however,*" (Edinburgh) "*the practice*" (which he advocates) "*has made but slow progress; and as for large bleedings, as they are called, that is, from twenty to forty ounces, they are still viewed with rooted aversion, by the greater part of private practitioners.*" Dr. James Johnson, in reference to the fevers of India, says, "*bleeding, I know, is seldom employed; but I can state, that three other surgeons on the station, beside myself, had recourse to venesection, in the fevers of India, with the greatest benefit.*" "*This is a small band, opposed to the host of antiphlebotomists,*" &c. &c. "*In the first place, a great proportion of practitioners will be deterred from the use of the lancet, entirely by the current of prejudice.*" Precious confessions these, even from the very advocates of the practice! It is a remarkable fact, that almost all the noise and clamour in the world, arise from minorities. Those who follow the fashion *strictly*, are very few. This is precisely the case with our profession. A highly respectable proportion of physicians, are still men of common sense, and common honesty. They however attend to the business of their profession, without making any bustle. We do not know a single physician in Connecticut, who attempts to treat a case of low cool typhus, upon the plan of Glutterbuck, Armstrong, Welsh, and Beddingfield; and we have the amplest testimony, that their practice is not generally adopted in England. Why our reviewers should take it so much in dudgeon, because we have exposed the absurdities of this class of writers, it is difficult to conceive. We have even the amplest proof, that one at least of these very authors, does not practice upon the principle; that he recommends.

ionable treatment of the epidemics of Spotted-fever, Pneumonia-typhodes, malignant Dysentery, common Typhus, &c. in the United States? Dr. Miller's able essay, had the reviewer studied it, might have given him some light on this subject. What is the truth with respect to the mortality from Consumption? The facts in support of our assertions on this point, are too numerous, and too notorious, to admit of question or denial. Even the avowed principles of our reviewer, afford a striking illustration of our statements; for, although he pretends to object to prescribing to the mere names of diseases, yet he seems to have no idea of deducing his indications, from any other source than the mere *names* of symptoms. In our work, we contended, in company with Cullen and his successors, (and we still contend, however unfashionable it may be,) that the latter method is full as absurd as the former. It is from *symptoms in their connexion and aggregate*, and not in an insulated form, that the only correct indications can be deduced. Thus, diarrhœa, in certain connexions, can be well treated only by emetics and cathartics; in others, these are injurious, and astringents and Opiates are indispensable, even from the access. Pain in the head, when in certain connexions, requires depletion and evacuations; when in others, it is greatly aggravated by these means, and requires a directly opposite treatment. Heat and dryness of the skin are sometimes relieved, but at others, are aggravated, by the internal use of the refrigerant salts. It is likewise well known, that in some instances, they are best relieved by the diffusible excitants; and at others, greatly aggravated by their use. Instead of absurdly pretending to specify what treatment, each symptom in the abstract requires, we insist that Cullen and his successors were right, in adapting their practice to certain aggregates, which they specified under the denominations of temperament, diathesis, type, diagnosis, &c. &c. and which, following them, we have detailed, under the same heads (except that we have no essay on temperament,) with greater minuteness, than is any where else to be found. We have repeatedly stated, that all these aggregates or groups of symptoms, are always to have their due weight, in the regulation of practice; and though we have no distinct essay upon diagnosis, yet this is a subject that runs through our whole work. In one place, where this is enforced, we illustrate by stating the well known fact, that distinct Small-Pox, and active acute Rheumatism, in proportion to the degree and intensity of those symptoms, which mark diathesis, type, stage, &c. require a much more rigorous antiphlogistic regimen and treatment, than Measles and Gout; and yet, our reviewer has the hardihood to say, that we almost entirely neglect the distinguishing symptoms of each disease. Had the critic considered our work as a whole, as we so evidently intended, he would have had still less pretext for his cavils. He would then have found all our important principles illustrated, in their application to the diseases, of which we have treated. That our review-

er, who would wish to be a popular writer, should so often and so strenuously object to this method of deriving indications, and should, either designedly or unconsciously, express such ignorance of the correctness of this method, is conclusive evidence, of the state of facts on this subject, at least, in our own country.

We have repeatedly anticipated the critic in stating, that the auxiliary branches, and even surgery, were very greatly improved; but we deny, that the pure practice of medicine, especially in fevers, is likewise improved. The attention, at this day, is distracted by minute and trifling circumstances, without any correct view of the whole, in their connexion. These opinions are brought up against us by the reviewer, as if they had been hitherto unheard of, and were exclusively peculiar to ourselves; but we maintain, that the same conclusion has been adopted by several of the ablest authors, with respect to the genius of the present age, and we shall therefore dismiss this subject, with the following quotation, with which, (our critic to the contrary notwithstanding,) we perfectly coincide. "*The great vice of the present day, is a presumptuous precipitancy of judgment; and there is nothing from which the cause—of general knowledge has suffered more severely, than from that impatience of investigation, and that confidence of decision, upon hasty and partial views, which mark the literary character of an age, undeservedly extolled, for its improvements in reasoning and philosophy.*"

We are repeatedly accused of having an hypothesis, that every disease is asthenic. We deny, however, that we have any hypothesis at all, and least of all, one of this sort. We expressly recognize, and even specify the opposite cases, and we in fact combat the fashionable opinion, that it is immaterial, whether such cases are treated by depletion and evacuations, or by oil of Turpentine, and other articles which are supposed to be stimulants. This absurd notion, when contrasted with the prevailing doctrine of late writers, that Typhus is to be principally treated by depletion and evacuations, may be considered as a glorious specimen of modern improvement and discrimination! In no instance do we assert, that a disease is asthenic, without showing that it has that assemblage of symptoms, which constitutes that diathesis. Had we even employed the terms sthenic and asthenic, nervous and putrid, &c. in a new sense, (and we contend we have not,) we have been so minute in the specification of what constitutes these states, that there can be neither mistake, nor misunderstanding. If in fact, we are in an error, in our decision respecting the asthenic character of every wide-spreading and devastating epidemic, except of the Exanthemata, we should feel under great obligation to our critic, to point out a single well attested instance to the contrary. For this purpose, he surely will not have recourse to such epidemics as the pestilence of Athens, the Plagues which spread terror and desolation in Europe, in the time of Boccace and Petrarch, the Sweating-sickness in



the days of Henry VII., the Pestilence which swept off the Indians of Massachusetts, two or three years previous to the landing of our forefathers in Plymouth, the great Plague of London, the modern Plague of the Levant, the Cholera of India, or the Yellow-fever of 1820, in Savannah. Dr. Good, in his Study of Medicine, seems, though indirectly, yet very clearly, to be of the same opinion with us, concerning the typhoid nature of wide-spreading epidemics.

The critic says, "*although Dr. M. sometimes speaks of the indiscriminate use of bleeding, &c. as being the practice he condemns, yet it is obvious from the whole tenor of his work, that it is the indiscriminate rejection of them only, that will satisfy him.*" Had the reviewer studied to make an assertion, which should be the most completely in opposition to all the specifications, and the whole tenor and scope of our work, he could not have hit upon one so palpable, or have been guilty of a greater violation of truth. Our rejection of depletion, and the cautions against profuse evacuations, are expressly and repeatedly, and throughout the whole volume, confined to cases of deficiency or exhaustion of vital energy, while these measures are uniformly maintained to be indispensable in the treatment of diseases of an opposite character. But, let us examine the practice of that class of physicians, who so falsely bring this charge of indiscriminate practice against us, that we may see, whether it cannot be with the utmost truth, retorted upon themselves, without being in the least uncharitable.

Dr. Welsh, who appears to be at the head of an important institution in Edinburgh, says, that "emetics are dangerous in their operation,"—that "their operation is very uncertain,"—that "they have little effect in violent fever, and frequently occasion those who depend on them, to lose time in the mildest." He adds, "emetics we have never seen of much service, and of course, they have been but seldom employed." "Purgatives are likewise uncertain in their operation,"—"they require some time also for their full operation,"—"the patient was often in a state of convalescence, from the use of the lancet, even before a proper system of purgation could have been brought to act upon the bowels;"—and yet in another place he affirms, that "purgatives have always had the best effects; and if there were any mode of cure, that could with justice be substituted for venesection, it would be purgatives!" He says, "sudorifics have been but little tried at Queensbury house"—"the objections against diaphoretics have been stated so strongly by Sydenham and Cullen, that it appears useless to insist further on them here"—"they are uncertain, slow, and disagreeable, even where they succeed; they are unmanageable, and often disappoint the wishes of the practitioner; and above all, can always be most certainly compensated for, by the use of the lancet, which is by far the safest diaphoretic." To these remarks, we add those of Dr. James Johnson, on the same class of remedies. "Now that the principles which govern the perspiratory process. are bet-

ter understood, the long and endless *farrago* of sweating medicines is reduced to a few neutral salts, as Citrate of Potassa, Acetate of Ammonia, accompanied occasionally with small doses of Antimony." "These, with cool diluent drinks, are the only safe or salutary diaphoretics in fever." We would barely observe, that if these are all the diaphoretics, which are at present employed by these gentlemen, in typhoid fevers, we are not at all surprised, at the result to which Dr. Welsh has arrived.

Dr. Welsh, on the subject of cold water, says, "as to the cold bath," (inclusive, as appears, both of affusion and sponging) "it may be remarked, that very seldom indeed, has it appeared to be of any service, and of course, it was little employed."—"Every one of the good effects supposed to be derived from the use of the cold bath in fever, can be procured with much more facility, certainty, and safety, by a free use of the lancet." He says, "with regard to Mercury, I doubt extremely, whether it has any effect at all, in diminishing fever; but as *nobody* believes that it cuts it short," (this seems to be at variance with the infallibility of *several* of our reviewers) "and the evidence in its favour, is so eminently ambiguous, I hope I shall be excused, if I pass it over, without further discussion." Dr. Welsh continues, "our readers, by this time, will be able to guess, that we do not lay much stress on the other remedies," (beside blood-letting) "which have been employed in fever." "In short, the activity of the remedy we treat of," (blood-letting) "compared with the others, is so preponderant, that I should not have encroached upon the reader's patience, by this comparison, short as it is, did it not seem proper, that prejudice should have an opportunity of viewing itself in various directions," &c.

As respects the indications for this universal remedy, *bleeding*, Dr. Welsh informs us, that he bleeds to abate fever, change type, and shorten duration; to lessen mortality, protract life, and promote convalescence; to mitigate heat, uneasiness, or pain; to remedy all irregular action, and local affection, such as spasm, subsultus, singultus, delirium, coma, ischuria, dysury, constipation, cuticular constriction, oppression, and congestion; to change temperature, and lessen the frequency of the pulse; to calm respiration, diminish thirst, and improve appetite, to check nausea, retching, and vomiting; to assist catharsis, restrain diarrhoea, and promote sleep. He says, that neither youth, nor old age, nor the long continuance of the disease, nor the hopelessness of the case, are any objection, or impediment to bleeding. Indeed, he considers it as of the greatest benefit, in the most hopeless cases; but its utility, he thinks, depends almost solely, on its being practised very copiously. He asserts, that it may be employed, not only with impunity, but with great advantage in fevers, to five times the quantity, that has been heretofore believed admissible. The benefit of bleeding, he thinks, is almost always in direct proportion to the quantity taken. His rule is, to bleed till relief of all



symptoms, or some injurious effect takes place. Its past failure, he says, has been owing to deficiency in the quantity taken. Excess, he supposes, is attended with little harm. Less than sixteen or twenty ounces, from an adult, he affirms, never does any good. He again inculcates, that strength, fulness, frequency, and sharpness of pulse, always indicate it, and weakness, quickness, and irregularity, are no sort of objection to it! Coma, whatever may be the accompanying symptoms, is said always to indicate the freest depletion! Dr. Welsh gives a specimen of the extent of his bleeding, in some of his cases.

CECIL SMITH, aged 25, he bled 136 oz. (more than a gallon) and applied ten Leeches, during one attack.

CASE GROVE, aged 24, he bled 110 oz. (about seven pints) 34 Leeches.

JOSEPH BURKET, aged 28, he bled 108 oz. 12 Leeches.

WILLIAM MARSHALL, aged 18, he bled 104 oz.

JANET NEILSON, aged 18, he bled 102 oz. 20 Leeches, &c. &c. and all this in Typhus!

In reference to the above abstract from Dr. Welsh, we shall take the liberty (however we may be accused of illiberality by our critic) of adopting a judicious remark of Dr. James Johnson, himself an advocate for very profuse bleeding. "*Indeed, we have frequent occasion to deplore the ultra-depletory mania, which has seized some weak brains of the present day, and much fear it will bring a character of rashness and danger, on a most important remedy, when judiciously managed.*"

Dr. La Roche, in Philadelphia, in a case of Colic, in which he evidently mistook the irritation, that almost every step of his process had a tendency to produce, for inflammation, took at least five quarts of blood from his patient, to say nothing of the most unparalleled evacuations from the stomach and bowels.

Dr. Watson, of Pennsylvania, in the case of Mr. Hale, which was subacute Pneumonia, or perhaps merely Catarrh, bled his patient every day, for at least nine days, if not more, till he was so reduced, that he was unable to change his position in bed. After this, he was kept forty-two days under a mercurial course, during which time, he took a large quantity of Calomel into the stomach, at the same time, having it rubbed into the mouth and gums; and having applied externally, by friction and bandages, thirty ounces of mercurial ointment, which consisted of almost equal parts of the blue oxyd and lard. Subsequent to this, he was salivated seventy-two days. From 8th April to 11th May, the daily quantity of saliva discharged, is specified, amounting to a hundred and thirty-nine pints, or more than seventeen gallons. From further statements, though the exact quantity is not mentioned, it is unquestionable, that a moderate estimate for the remaining thirty-nine days, must make the whole amount to about thirty-three gallons, or more than a barrel! Our critic may still sneer, and plead ignorance to such facts, if he pleases. Surely, without

any very great criminality, we may be allowed to say, that this great cure was only a great escape.\*

The same physician details another case, in which he salivated a delicate female, more than four gallons; but this, in comparison with a barrel, is too trifling to be insisted on. The genius of Moliere, or Le Sage, would bestow a much more appropriate chastisement, upon the authors and abettors of such practice, than the best talents of any serious writer.

Hamilton on purgatives, is so universally, in every medical library, that we imagine, even our critic will not plead ignorance of its contents; and if we cannot be allowed to make any objections of our own, to the practice which this work inculcates, we humbly hope, that we shall be permitted, to adopt language from high European authority. "*Hamilton's recommendation of purgatives in Typhus, only proves what blunders, and absurd methods of treatment, the human body can, in certain circumstances, withstand.*" We say the same, concerning gallons of blood!!!

We might compile volumes from popular authors, and the common periodical works of the day, not excepting even the New-England Journal, of practice nearly as absurd. The editor will recollect, that for the sake of commending a popular work, he reviewed it twice; though the author was so indiscriminate, as to mistake the struggles of the dying for sthenic diathesis; and most of his deductions were made from a set of peurile, wanton, and barbarous experiments upon *living* animals, which are a disgrace to the age we live in. But why all this clamour, on one side only? What do gentlemen mean, by accusing us of libelling the whole profession,

\* Some of our friends, who are not much conversant with the writers of the last twenty years, or who are not in the habit of *summing up* such statements, were apprehensive, that we had used too strong expressions, in our zeal for exposing the rash practice of our opponents. Salvation by the barrel, and bleeding by gallons, startled them. We, however, contend, that no language can expose it, in too glowing colours. We claim it, not merely as a right, but we consider it as a duty, which we owe to humanity, and the age we live in, to make a public, solemn protest, against such indiscriminate, murderous practice. The more fashionable it is, the stronger is the obligation of every good man, to meet it with expressions of his most decided disapprobation and abhorrence. It was our endeavour to be literally correct in our estimates, and to venture no assertions, for which we had not the most ample documents. We have watched, ever since we have been in practice, the effects of indiscriminate bleeding, after casualties, *fits*, hysteria, &c. and in typhoid fevers, and other diseases, attended with a prominent deficiency of vital power, and we do not hesitate to say, that annually, two persons in a thousand, is a *very low* estimate, in the United States, of the victims of injudicious blood-letting. From twenty to thirty thousand, is the *necessary* result, for our population! If such is the proportion in Europe, it is evident, that the lancet slays more than the sword. We could specify abundance of *names* and *instances*, in justification of our *calculations*. The best things, when perverted, cause the greatest evils. Let no one, therefore, misrepresent us, as being enemies to the proper use of the lancet. We have never questioned, that it is the sheet-anchor, in every violent phlogistic disease. We inculcate its vigorous employment, again and again, in various places in our work, as any one who will take pains to read us, will readily see. This will show the public, with what cautions, they must receive the false allegations, of a set of superficial, partial, and malignant reviewers. To understand the nature and tendency of our work, "*lisez vous-mêmes et jugez,*" read it yourselves, and then judge.

and of using unpardonable asperity of language, in reprobating such practice? Why are they so sensitive, when any attempt is made, to expose such errors. Why after all this, is it such an inexpiable crime, to maintain, that the fashionable practice of physic in febrile diseases, when compared with that of Cullen and his successors, is deteriorated? Why do many of the Journal writers feel such a horror of stimulating practice, in the exhaustion of Typhus, and at the same time, remain so callous, in reference to such a waste of all the powers of life? Why do they continue to publish such preposterous practice, without note or comment, and manifest such violent opposition, to every contrary doctrine? Why do many Physicians, whose private practice is substantially the same with ours, still publicly censure the very method, which they daily employ, and pretend to approve and recommend a course, that neither their consciences, nor their reputation in business, will allow them to execute? Why is the whole fry of petty writers and compilers, so clamorous, in misstating the matter; and cavilling at the manner, of what they have either not read, or are Incapable of understanding? Will it be pretended, that the works to which we object, are the most proper, to teach students practice? If so, why are we not met with opposite facts, instead of scurrility and abuse? After all this, we may well ask, are we not amply justified, in our assertion in our preface, that "*it is of immense importance, that the present confusion of the practice of physic, in the treatment of acute febrile diseases, should be known,*" and that "*some author is needed, of so much independence of mind, that he is not afraid to hazard his popularity, by publishing a true statement, of the most important facts.*" We again take occasion to observe, that all the statements in our work, in regard to this subject, are *literally* true, and are capable of being substantiated, in the most unequivocal manner. Indeed, we can actually refer to cases, in support of every important circumstance, which we have mentioned.

There is but one point more, in the review in the New-England Journal, which deserves notice, and that is the declaration, that "*the proper period for bleeding, in all cases, where there can be the least doubt, as to its propriety, is always short,*"—and "*we do say with much confidence, that the cases are frequent, where if bleeding be not practised, in the beginning of the disease, no subsequent efforts can repair the mischief, which the omission has caused to the system, so as to give the patient an equal chance for a favourable recovery.*" These assertions we hold to be altogether groundless. In a genuine and pure phlogistic disease, the progress is not so rapid, the range of depletion is not so limited; it almost always extends to two or three days, and more frequently, beyond this period. We have ourselves bled, with decided benefit, (when depletion had been previously omitted, because it was before considered as doubtfully indicated,) as late as the ninth day of a sthenic Pleurisy. This



is a point, to which our attention has long been turned, and we are confident, that in those cases which are alluded to, by the critic, depletion should always be omitted entirely, as they may be treated much more effectually and safely, by other means. Whenever blood-letting succeeds, (and we agree in our Essays, that it occasionally does) in such a case of disease, it is upon the principle, that drenching with hot Spirit and water, occasionally breaks up a Synocha; and the measure in question, is attended with equal disadvantage, and equal hazard, provided it fails. Our reviewer, evidently has not read us with sufficient attention to discover, that we have amply discussed this very subject, in our work. People in general, do not know upon what point, the question concerning the propriety of bleeding turns. In cool Typhus, or regular Nervous-fever, cool Pneumonia, cool Cynanthe-maligna and Croup, cool Yellow-fever,—in a word, in nineteen twentieths of the cases of the febrile diseases, which for a number of years past, have prevailed, in our section of the country at least, there cannot possibly be any dispute, among rational men. In such cases, efficient depletion is always disastrous, and often fatal. In the whole of these cases, there is evidently, a greater or less deficiency of vital energy, from their access, to their termination. It is only in the hot cases of febrile disease, that there is any room for controversy. In Fevers and Phlegmasiæ, attended with the genuine Synocha of Cullen, when the symptoms are urgent, all the world (the modern exceptions, in point of number, are too insignificant to deserve mention,) agrees, not only to the propriety, but even to the necessity of efficient depletion, and evacuations. Such diseases do not occur *among us*, at the present day. But there is a kind of fever, called Synochus, by Cullen, (the moderate subputrid-fever of our work; said to be the most common fever of Great Britain,) which though it is not attended with the urgent symptoms, of the more highly putrid-fever of authors, has yet daily exacerbations of heat, for a considerable time. These are the cases, about the early management of which, there is so much controversy. This is almost the only point, on which we are very essentially at variance, respecting depletion merely, with *most* of the popular authors of the day. In the Essays on type, Yellow-fever, and more particularly, in our analysis of Dr. Miller, this subject is amply discussed. As the critic has not opposed our doctrine, by a single fact or argument, but only met us by mere assertions, we shall not here beat the air, or attempt to fight the battle over again. It is an important question, and we sincerely recommend those parts of our work, in which it is examined, to the studious attention of every member of our profession. We shall only add, that the cases to which our critic refers, are evidently the violent and rapid instances of this sort of fever.\*

\* The Boston critics, by their publication in the North-American, have appealed from the medical profession, to the literary and scientific part of the communi-

A few remarks only, will suffice for the North-American review. We can assure the writer, that we most cordially concur in the sentiments contained in his preamble; and we have inculcated them, throughout the whole course of our work; that "*we have no medical system to support or overthrow*," that "*we are wedded neither to the lancet and hot water, on the one hand, nor to Opium and Brandy-toddy, on the other*," that "*we believe in all, or either, in their proper times and places, and are ready to admit, that there is a proper time and place for each*." From Boerhaave, we have adopted as a fundamental maxim of our work, "*nullum ego cognosco remedium, nisi quod tempestivo usu fiat tale*;" and we expressly insist, that "*true medication is not a bed of Procrustes, to the length of which, every person must be either stretched or shortened; but on the contrary, it is a couch adjusted to the dimensions of the patient*." Though our principles are "*nullius in verba magistri*," and we are unfettered by the dogmas of any medical school, yet with justice, we claim a place among that class of "*writers to whom the designation of VITALISTS more strictly applies; a sect which may be characterized, as professing to deduce all their opinions, from the correct observation, of the essential powers, and faculties, of the living body, without the assumption of any hypothetical principles, or the introduction of any agents, for the sole purpose of explaining phenomena*."

The North-American has evidently had better success, in obtaining some glimmerings, of one of the objects of our work, than his brother in the New-England Journal; though in common with him, he has mis-applied

ty. We have no question, that this reference will ultimately produce a good effect, by calling the attention of gentlemen of general reading, to the state of the practice of physic in our country. It has already caused an inquiry after our work, among this respectable class of citizens, who have no prejudices to subserve, no interests in the contest, except what concern the lives and health of themselves and others. More than one gentleman, who is not of the faculty, indignant at the course the reviewers have taken, have volunteered their able pens in our defence. Though we intended our work exclusively for Physicians, (or we should not so generally have clothed it with technical language,) and especially, for those who had practised in malignant epidemics, yet it contains much to interest the general reader, and repay him for his diligent attention to the weighty matters it treats of. Our Essays on experience, stage, type, crisis, yellow-fever, and pneumonia, the remarks upon the undue preference to surgery, the subjects of blood-letting and inflammation, and more particularly, the plan for the resolution of regular fevers, are of the highest importance; and with a little attention, every gentleman can make himself acquainted with the outlines of our plan. By reading the reviews of the Essays, (which we also recommend, in connexion with our work,) he will see, what are the feelings, talents, and integrity of those, who pretend to direct the public taste and judgment, upon subjects connected with the medical profession. He will also discover, that our critics have assumed to themselves a task, to which, they are both morally and intellectually incompetent. In thus referring the subject to gentlemen of general literature, our reviewers, though unintentionally, will, without any doubt, call the attention of the public to the present deranged state of the principles and practice of medicine. It is the North-American, (not the authors) who has made the appeal. We, however, have the greatest respect for the board of their choice, (though we in general disapprove of such references of medical questions,) and repose the highest confidence in its decisions, not doubting, that before this tribunal, we shall have the fairest and most impartial hearing.



our strictures, respecting the indolent and the ignorant, the indiscriminate and the rash, to the whole profession. We do sincerely hope that our labours will have some "*tendency*" to destroy "*public confidence*" in such practitioners, even though it may affect "*their reputation, their very livelihood.*" With respect to the accusation, that we "*deal too much in assertion to be very convincing,*" we have only to say, that the bare results of observation and experience, must always be expressed in assertions, which we contend, can never receive much support, from theoretical reasoning. The truth and validity of such assertions, can easily be confirmed or overthrown, by a repetition of the trials and experiments, which first led to them.

Surely the critic cannot sincerely imagine, that it is of any consequence, so far as respects the accuracy of the facts and principles, which the work contains, or its utility to the public, whether the Boston reviewers ever had, or had not, heard of the name of its authors.

From a determination to sneer, the critic totally misrepresents what we have stated as fact, concerning the extent of our opportunities for experience. Our friends well know, that our practice has not been limited, to the City, (or incorporated village,) or even to the township of Middletown. We expressly state, that we have had *several* epidemics, within the circuit of our practice. Besides, we have each of us practised for years, in other towns in Connecticut. But this is the last subject, upon which we are disposed to insist, as we contend, that length of years, and number of cases, do not necessarily confer *true* experience. Dr. Hunter's four thousand cases, are no proof, that he was an eminent practitioner in fever. But our reviewer allows, that our "*method is a very good one, in many cases of typhoid fevers,*" though he seems to take much credit for discovering, what we have again and again declared and inculcated, in various parts of our work, that it is not "*the only one admissible.*" He even appears to have a higher opinion, of our general method more particularly as relates to the use of Calomel, than we have ourselves; for we have been very particular in specifying numerous exceptions, in which we consider it as inadmissible.

The critic's sneer at our insisting so much upon what is contained in the maxim, "*morbo incipiente, currite medico,*" does not merit any serious notice. His insinuations, that it would be more for the emolument of the profession, were this maxim universally adopted, shows him to be totally ignorant of the subject; as every practical physician knows, they are entirely unfounded. Without doubt, the greater half of the business and profits of the profession, arises entirely from popular tampering with medicine, and injudicious delay in applying for regular medical advice. Let us hear the New-England Journal upon this subject, which surely *our critic in the North-American Review*, will not attempt to controvert. "*An hour lost at the commencement of a disease, will often prove to be*

*a day lost, in the time of its termination."* "There is no such thing as getting over the evil consequences produced by a delay, in the application of the proper treatment." See N. E. J. Vol. 12th, p. 38th. How do reviewers disagree among themselves !

The palpable misstatement, (to use no harsher or more appropriate term) that we have "*never failed in more than one instance, to cure Consumption, when a fair opportunity has presented itself;*" ought not to pass, without the severest animadversion. We expressly confined our statement, to *two* of the several varieties. None but a reviewer, who had the vanity to imagine, that he had influence sufficient to suppress a work, could have dared to hazard such an obvious perversion of an author's words and meaning.

In reference to the pulse, we have invented no new terms, but have merely adopted such, as we found in common use. However the critic may dislike the denominations, had he quoted their accompanying definitions and explanations, we claim, that no room would have remained, either for cavil or objection.

With respect to our Essays on Yellow-fever, we do not pretend to any very extensive practice or experience, in the disease which is their subject; yet we still insist, that thirty or forty cases, which occurred in 1820, and former years, the origin and progress of most of which, we had opportunity to watch with the minutest attention, may contribute somewhat to elucidate the history of this important disease, and do assuredly entitle us, to contribute our mite, to the general mass of facts upon record. Not having any theories to support, in relation to this disease, we trust we shall be acquitted, by every candid reader, of any disposition to conceal a single circumstance; and we take this occasion to declare, that we have stated every thing, of which we could obtain any knowledge, whichever way it might tend. The only point, on which we have drawn any inference, as respects its cause, is, that *with us* it was not of domestic origin. We expressly waived the discussion of this point, as it concerns our southern cities. This is all that we claim, from the history of the disease in Chatham. Whether our cases were all Yellow-fever, the medical public will decide. We insist, that they were all of a common character, and differed *materially*, from any of our ordinary fevers. We however believe it in vain, to contend with those, who have not frequently witnessed the disease. We cannot too often insist, that *no description, of Yellow-fever, Spotted-fever, malignant Pneumonia, malignant Dysentery, malignant Typhus, and every other malady, that from its very access, is attended by a dangerous deficiency of vital power, can ever be realized, by those who are not conversant with such cases.*

We will say one word of the last Essay, which our critics have scarcely condescended to notice, although the subject is a disease, (Pneumonia typhodes) which has spread desolation through our country, and which

appears to have confounded physicians, as much as Yellow-fever itself. Had this Essay, we insist, received any attention, it would have been found to contain a minute illustration, of most of our ideas, concerning principles and practice, and would have been found to anticipate and obviate most of the cavils and objections, in either review.

With respect to the critic's remarks upon our spirit and manner; his *mis-application* of our strictures upon the exceptionable members of our profession, to the whole body; his objections to the originality of our *method* of curing fevers by resolution; the insinuations, that our practice is indiscriminate; his accusation, of our want of respect to particular venerable names;—we have already treated upon these subjects, in our reply to the review in the New-England Journal, and even had repeatedly anticipated them in the work itself. Nearly "*every ignorant objection, and every rash and petulant observation, has been taken up, and commented upon.*" Even every insinuation, that can be deemed by any one, at all worthy of observation, has now been directly, or indirectly, remarked upon. In fact, we believe an apology is due to our friends, for the notice of so many trifling cavils, which it would be an insult to their common sense, not to suppose too palpable, to require refutation.

Upon the perusal of these two criticisms, it struck us instantly, that their authors had just been reading the Edinburgh Review, upon the subject of periodical publications, and had availed themselves of most of the unfair means, which are there so minutely described, as the resort of those booksellers, and others, who *combine* to prevent the circulation of a new work, that opposes their prejudices or interests. The Edinburgh writer, while reprobating such arts and practices, could never have anticipated, that his details would furnish the means of perpetuating the management, which he so properly and forcibly censures. The reading public of America is, however, very differently situated, from that of Europe. Here, authority is of no weight, any further than it is just. Every man prides himself, upon thinking and acting, according to his own judgment. Our critics have adopted neither the plan of analyzing our work, so as to afford their readers a summary of the principles which it contains, followed by comments of their own, on its correctness and tendency; nor that of attempting to write better essays of their own, upon the same subjects;—the only two methods, as far as we know, by which a critic can either instruct, or enlighten the public. The New-England Journal, it is true, makes numerous detached extracts, but they are done in such a manner, as to afford not the least accurate conception of the work; and many of his misrepresentations are so gross, as not to be accounted for, upon any other principle, except the certainty, that he had never read it with such attention, as to discover its most obvious meaning. No man, from his statements, can form the most distant idea, of the real scope and tenour of the volume. The North-American professes not to care much about the matter, or rather, he seems to agree with us, in most of



the essential points; he only cavils at the manner, without entering minutely into any of the subjects.

In one respect, our work differs from most other professional treatises. The authors show the greatest ardour for the improvement of their profession, and they strenuously insist, that physic should not be a mere trade, that it is necessary for the medical faculty, to be honest and conscientious men, that they should be highly sensible of their moral accountability; and every practice, that has a contrary tendency, is reprobated, in the strongest terms. Is it possible, that such sentiments can offend these gentlemen reviewers? Our work is a collection of facts, and deductions from facts. Not one of our assertions has been disproved, or shown to be incorrect, by either of these critics. They appear to have been fully sensible, that it was invulnerable to this method of attack; or at least, they have shown themselves to be both morally and intellectually incapable of such an undertaking. Neither of them has hazarded the proposition of any better plan, or ventured to recommend and support an opposite method of practice, in the *particular* diseases of which we treat. They seem merely to have aimed at forestalling public opinion, to have attempted to make the work unpopular,—and they have endeavoured to prevent an investigation of the important principles which it contains. We never knew, in our country, of an instance, in which all the principles and ends of genuine criticism, were so openly and wantonly violated, in a periodical work, that made any pretensions to respectability. What work that treats of subjects, in which all mankind have the greatest interest, was ever before attempted to be silenced, by captious exceptions to its manner and spirit? When was there ever such railing at an author, without any serious attempt to refute his doctrines and facts?

On the whole, we cannot but believe, that by every impartial man, these reviews will be considered to be the highest possible commendation of the accuracy and fidelity of our work. “L’on peut acquérir une bonne réputation de deux manières; lorsque les honnêtes gens disent du bien de nous, ou que les coquins en disent du mal. L’on doit, cependant, s’attacher de préférence à la première, parce qu’on y trouvera l’autre, sans le moindre doute. Quand les coquins disent du mal de nous, c’est le plus grand bienfait que nous puissions en recevoir; et de plus, c’est le seul service qu’ils nous rendent, pour rien.” “*All that art could devise, all that malice could invent,*” has not hitherto been able to point out a single inaccuracy in our statements. Our countrymen are an inquisitive people, who will not found their judgment upon the mere *ipse dixit* of an anonymous reviewer. Here, the mind is not so enslaved, that two or three critics, can prevent the reading of a new work, or suppress the circulation of its doctrines, when they are grounded on reason and truth. With the public, the only question is, whether our principles and inferences depend upon facts. If they prove to be true, when tested by experience, they will finally prevail, in defiance of all opposition.



## NEW-YORK MEDICAL AND PHYSICAL JOURNAL.



We "turn with pleasure from that barren waste, in which no salutary plant takes root, no verdure quickens."

After tiring our readers and ourselves, with endeavouring to trace the various "ramifications of fraud, error, and delusion," which have been exposed during this examination, it is quite a relief to the mind, to be able to reverse the picture, and to notice briefly a critical review of a very different stamp. We have still many able physicians; nor is there a deficiency of candid and impartial reviewers. Two of the most respectable editors of our country, have given the strongest testimony in favour of the general principles and practice, which are recommended in our work. They do not allow some doubts, that they entertain with respect to the *manner*, to bias their judgment, and prevent them from giving the most ample credit to the all-important *matter* which it contains.

In No. 7 of the New-York Medical and Physical Journal, edited by Drs. Francis and Beck, our readers will find a review of the Essays on Fevers, which is written in a very different spirit from the criticisms, which we have just been considering. The urbanity and candour of these gentlemen, afford a striking contrast, to the rudeness and partiality of the Boston reviewers. We are happy to find, that these physicians perfectly agree, as to the correctness of our principles, in application to *our* diseases. Our remarks, upon the undue preference which is commonly given to surgery, we think, are much enforced by an extract, which they have made from a very respectable author. Though they *evidently* do not appear to have read us with *minute* attention, or to have distinguished with *sufficient* accuracy, the *peculiarities* of our method of using Calomel, yet they entirely coincide with us, as to its general effects, and they remark, "*we think it but justice to Dr. Miner to say, that his treatment in general seems to have been very successful, and to be regulated by sound practical views;*" and, that the Essays on Pulse, Type, Crisis, and Stage of Fevers, "*are full of practical information, and merit attentive perusal.*" They have even strengthened our remarks on Experience. They further recommend to their readers, "*an attentive perusal of the volume, and the adoption of Dr. Miner's practice, in ALL CASES that are asthenic and attended with exhaustion.*" They conclude their review with the following remarks, in explanation of the strictures, which they have made.

"We hope, that neither of these very intelligent physicians will misconstrue our motives, in thus freely animadverting upon their opinions of the

practice of others. They are mistaken, if they suppose for a moment, that we undervalue their labours, or mean to throw any reflection upon the intelligence, the ability, and the vigour of thought, which they have displayed in the composition of these Essays. With the exception of the one fault, to which we have just adverted, [this exception, the authors conceive, they have satisfactorily removed in their reply to the eastern critics] we must express our *unequivocal approbation* of the general spirit, and the manly diction, which pervade their writings, and which, we hope, will not fail to inspire our professional brethren in the country at large, to imitate the example of our enterprising friends.

“On taking leave of this interesting volume, we beg permission of the authors, to urge them to the promised continuation of their Essays. We have long known Dr. Tully; and from the ardour and talent which he has evinced, on various occasions, for scientific investigation, we augur most favourably and pleasingly from the continuation of his labours.”

Middletown, (Conn.) Dec. 24, 1823.



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